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THE following communications were read:—

I. Comparisons of the right ascension of *Polaris*, as obtained by observation, with that given in Bessel's catalogue for 1831; and observations of λ *Ursæ minoris* and 51 (Hevelius) *Cephei*, made with a 5-feet transit at Blackheath. By Mr. Wrottesley.

II. Mr. Sheepshanks gave an account of the mural circle, with the methods of using it hitherto followed, and a description of the state and defects of the circle at the Cape Observatory.

The Council of the London University were kind enough to lend their beautiful model of a mural circle, which was placed upon the table; but a sufficient idea of the construction of the instrument, omitting the nicer adjustments, may perhaps be obtained from the following description. It consists of a circle, not very unlike a cart-wheel, attached by its centre and at right angles to one end of a long hollow conical axis. Upon this axis are two well-turned fittings, one near the circle, the other at the further extremity, which work in collars* firmly fixed at the front and back of a massive stone wall or pier extending beyond the circle. The telescope is moveable upon the *face* of the circle, on an axis concentric to and working within the axis of the circle, and is held in any required position by strong clamps at the object and eye end. The divisions are cut on the edge of the circle (similarly to the milling upon our coin), and the reading microscopes are fixed against the pier. In large instruments there are generally six microscopes, at equal intervals of 60°. To relieve the bearing upon the collar, a pair of friction wheels support the axis immediately behind the circle, and thus a smooth and easy movement is secured. The friction wheels are suspended from one arm of a scale beam. The limb of the circle is turned upon its own axis (as on a lathe), upon which it is also divided after the method described by Mr. Troughton

* In the Cape mural, Y's were adopted instead of collars, with the expectation that the instrument would thus keep its meridian better. An objection has been made against this construction, that the circle when turned round would probably ride up the inclined planes. This would not seem to have been the case in the Cape circle, as the instrument, though very much deranged, gives at all times the same readings with each microscope at the same place; but the objection, as will be seen hereafter, is of no weight. The *riding* may easily be detected; and would not, on any reasonable supposition, affect the results.

in the *Phil. Trans.*, so that the divisions must be concentric with the axis. In every part of the instrument, of its pier and microscopes, the utmost firmness and stability have been aimed at.

For the purpose of setting the axis horizontal, two adjustable dots are fixed on the front of the telescope towards the extremities. A plumb-line is suspended from the top of the pier, and made to bisect the *images* of these points when the telescope is vertical; the instrument is then moved round 180°, and the plumb-line again applied and made to bisect one of the dots. If it also bisect the other, the axis is horizontal; but if not, the difference is to be halved by raising or depressing the further end of the axis, which is furnished with adjusting screws. The same operation may be performed, or the error measured, if the transits of stars be observed directly and by reflection.

The simplest mode of placing the line of sight at right angles with the axis would seem to be by employing a second or collimating telescope, as is practised at present at Greenwich with the transit itself. Let the centre wire of the circle telescope bisect a well-defined mark in the horizon, to the *South*, for instance; and let a telescope, furnished with cross wires, and moving on a horizontal axis, be placed to the *North* of the circle, in the same vertical plane with the mark and the circle. Then, if the cross wires of the collimating telescope be made to bisect the mark, and the circle be turned round so as to look down the collimating telescope, the cross wires should be bisected exactly as the mark was. One half the error, if any, is to be corrected by screws working in the wire plate of the circle telescope. Lastly, the centre wire, which now describes a great vertical circle, is made to pass through the pole, by observing the transits of circumpolar stars at the upper and lower culmination, or by any other method. The adjusting screws for this purpose are applied at the extremity of the axis.*

Objections have been made against this construction of the *declination circle:* one or two of which may be briefly noticed. The first is, that the circle being supported on one side, has only half an axis. The mischief to be apprehended from this, is the bending of the axis outwards and downwards from the point where the friction wheels bear, which would cause the plane of the instrument to be no longer at right angles to the axis, and therefore not in the meridian. The telescope would describe such a circle as a transit with an error of inclination. When, however, the strength of the axis (which is seven inches in diameter), and the shortness of the lever at which the weight acts, is considered, it is evident that the flexure must be very small; that this is remedied, so far as its mean value is concerned, by giving a corresponding inclination to the axis; and that the effect of such bending would not be sensible in measurements of declination except in stars very near the pole.

* If it be intended to use the instrument as a transit, these adjustments must be performed with scrupulous exactness; but a considerable error will not sensibly affect the accuracy of the results, when the instrument is employed in merely measuring declination. A third telescope will supply the place of a mark.

Another, and a better founded objection has been urged, which is, that, as the divisions are on the edge of the limb, they are not only affected in magnitude by temperature, but are brought nearer to, or removed further from, the microscopes, which causes an alteration in the *run* of the micrometer, and may produce considerable error. If this be so, the fact is easily ascertainable by trying the *runs* after considerable changes of temperature, and the error may be remedied by correcting each microscope, or their sum. Perhaps it would be prudent, at all events, to verify the runs from time to time by bisecting both the upper and lower divisions, as was frequently done by the Astronomer Royal when Troughton's mural was first erected.

Whatever weight, however, may be attached to these, or similar objections, would seem to be much more than balanced by the advantages peculiar to the instrument; viz. the superior firmness of its construction, allowing the use of larger dimensions with the application of higher optical power than can safely be employed in any other form of the circle; and the extreme steadiness of the microscopes, which continue unaltered, if not struck or meddled with.

When the mural circle was first erected at Greenwich, the Astronomer Royal, after many trials, adopted the mode of observation which is fully explained in the *Greenwich Observations*, Vols. I. and II. The details of this method have not been generally understood; but the principle may perhaps be explained thus. A certain number of stars are selected to form the standard catalogue, and their places in $\mathcal{R}$ and N.P.D. *assumed*. Let us further suppose the difference of the assumed from the *real* position of each star to consist of two parts, one, x, common to all the stars, or the mean of the differences between the assumed and *required* catalogues, and the other, the error proper to the individual star. Now, if the *whole* of these stars be observed on any day, in N.P.D. for instance, the difference of each observed place from the assumed place will be the index error of the instrument according to that star, and the mean of these index errors will be the real index error of the instrument $\pm x$. By using this quantity as the index error, the assumed place of each star is corrected for the second or individual portion of the error; and all that is further required is to ascertain the value of x. This increases the N.P.D. of stars above the pole, and diminishes that of stars below the pole, or *versâ vice*, and thus the value of x is equal to half the difference between the N.P. Distances of the same star above and below the pole. The method is less perfect when fewer stars are observed; but it is obvious, that when the observations of many days are employed, and nearly in the same number for each star, the result will be the same. The resulting catalogue is to be considered as *standard*, when the index error by every star above and below the pole is the same, allowing for errors of observation.*

* The mode of forming a catalogue in $\mathcal{R}$ is precisely the same, except that the mean error of the assumed places is to be obtained from observations of the

In this way the observations at Greenwich were conducted for some years, until a second mural circle was erected, made by Mr. Jones, and originally intended for the Cape Observatory. It is evident, that in the preceding method, the steadiness of the instrument with its microscopes, and a knowledge of the *variation* of refraction at different altitudes, and under different atmospheric conditions, are implied; and it is probably to avoid these assumptions that recourse has been had to the new system, which consists in obtaining double altitudes *at the same time* by using a reflecting surface of mercury. The circles are placed opposite to each other, and in every respect under the same circumstances. Let us suppose six or seven stars observed at each instrument in the same position, for instance by direct vision; and further, that the mean of the readings by Jones's circle exceeds the mean of the readings by Troughton's by 10″. If a star be now observed by Jones's *directly*, and by Troughton's by *reflexion* in a trough of mercury, it is clear that the reading of Jones' exceeds the reading which Troughton's would have shewn (if the star had been observed directly by that instrument) by 10″; or that if 10″ be subtracted from the reading of Jones', we may assume this for the reading of Troughton's circle, and suppose the star to have been observed by direct vision by the latter instrument: the difference between this *transferred* reading and that actually made is the apparent double altitude of the star.* In this method no dependence need be placed upon the stability of either instrument for any considerable time, and no knowledge of the refraction is required. In the opinion of the Astronomer Royal, it is a very sensible improvement upon the first method; but it can only be carried into effect where two observers can be appropriated to the circle.

At the Cape, as the pole is low and no stars visible in the day time within 10° of it, Mr. Fallows could not very safely apply Mr. Pond's first method, which implies, as is said above, a perfect knowledge of the difference of refraction of a star above and below the pole. He, therefore, observed the same stars on one day directly, and on another day by reflection, relying upon the stability of the instrument, pier, microscopes, &c. during this interval, and supposing the variation of refraction, at the same altitude, arising from pressure and temperature, to be sufficiently known. He thus obtained double altitudes from the observations of two days. To prove the steadiness of the instrument, observa-

equinoxes. The mean error in N.P.D. is similar to a wrongly-assumed situation of the pole, and that in Æ corresponds to a wrong place of the first point of *Aries*.

* The half of these readings gives the place of the horizon upon the circle, and the constancy of this place as determined by different stars on the same night is a test of the goodness of the observations and of the equality of the divisions of the instrument. When the horizontal point continues unchanged for a long period, the stability of the pier, &c. may be concluded. To understand the manner of practically carrying into execution what has only been briefly hinted at above, the reader is referred to the *Greenwich Observations*, where all the details are fully exhibited.

tions of some stars were generally made in the same position of the circle, and the constancy of such readings shew that no sensible change took place for several weeks together. It will be seen that the only danger to be apprehended is, that the whole pier should move in one mass, round some horizontal axis, and this may easily be verified in the manner stated above. A derangement in one or two microscopes, or a change in the position of the centre of the circle, cannot escape the notice of any careful observer, and may be ascertained or corrected with certainty.

A fourth method has been suggested, but not attempted, at least with quick moving stars. After setting the instrument pretty nearly to the place of the star, by reflection for instance, and clamping it, the microscopes are to be read off, and the bisection of the star performed by a micrometer attached to the eye-piece. The clamp is then to be released, and the star bisected on the micrometer wire by direct vision, the slow motion being given to the instrument in the usual manner with the tangent screw, and the micrometer wire untouched. The difference of the two readings will give the double altitude of the star. Experience only can shew whether this mode of observing can be practised safely and without hurry, by one observer, with an instrument upon so large a scale as a 6-feet mural. With small circles there would be no difficulty, and if they have an azimuthal motion, the micrometer may be dispensed with.

Before describing the kind of injury which seems to have been sustained by the mural circle at the Cape, it seems advisable to make some preliminary observations, which will shew the nature of the evidence from which certain conclusions have been drawn.

Almost every person who has examined a circle by reading verniers, or microscopes, placed at 180° distance, will have remarked, that the readings are discordant, or do not differ by a constant angle. This is particularly the case in circles divided by engine, and is easily accounted for, when we consider that, though the circle may be accurately divided, there is no necessary connexion, at least in the English construction of the dividing engine, between the centre of the divided circle and the axis round which it, or the bar carrying the verniers, ultimately moves.* No error is caused by a defect of this nature, when opposite readings are employed. If the circle be fixed, and the verniers revolve, which is generally the case in theodolites, &c., we may imagine a vernier to pass through the centre of the divisions which is always parallel to the existing excentric vernier. This hypothetical vernier, if the circle were well divided, would give the same readings at each extremity, one of which would exceed the neighbouring reading of the real vernier by exactly the same quantity by which the other reading falls short of its companion. The means of the two verniers would coincide, which is also clear, from the consideration that the sum of

* M. Gambey has provided for this in his engine, by making the axis of the circle to be divided the axis of his machine; this is not done in Ramsden's construction, which has hitherto been followed in this country.

the readings of parallel chords must always be identical. Again, if the circle revolve, and the verniers or microscopes be fixed, as in the mural circle, it is evident that the raising or depressing the centre will not alter the sum of the readings of opposite microscopes, if the position of the telescope, or of any diameter, continue to be parallel with itself.* Whatever is lost at one microscope is gained by its opposite. The same is practically true† of three readings at 120° distance. Of course, four and six microscopes (as consisting of two and three pairs) remedy any defect of centering, which is, besides, so readily separated from errors of division that no fear need be entertained on this score. In some cases a little excentricity may be even useful in examining and checking the work of subordinate observers, who may not be sufficiently intelligent to forge consistent readings.

Another proposition (easily shewn from the series which expresses the arc of an ellipse of small excentricity) is, that if an equally divided circle be from any cause changed into a slightly elliptic form (without altering the length of its circumference, which continues to be a scale of equal parts); then, the reading of the ellipse by any microscope will be the reading of the original circle $+ m'' \times \sin 2\theta$, where m depends upon the extent of the derangement, and θ is the arc reckoned in the order of the divisions, and measured from the major axis of the ellipse. A little consideration of this expression‡ will shew, that two microscopes at 90° distance, three microscopes at 120° distance, and hence four and six microscopes at equal distances will give the same result, whether the original circle or the ellipse be employed; and therefore, that any injury to a circle which alters it to an ellipse without extending the limb, does no harm, if the proper number of microscopes be used, and in their proper places.

Again, if the plane of the circle be not placed at right angles with the axis, or, to use the workman's phrase, if the wheel reels and goes drunken, it would seem that the effect of this obliquity, when small, is precisely such as would be caused by supposing the circle to be elliptic, and would, therefore, produce no error, if the proper number of microscopes be used.

* Hence, a defective clamp, which alters the readings when applied, causes no error (since the bisection is made after clamping), unless the instrument slip before the microscopes are read.

† *i. e.* if the excentricity be so small, that, when divided by the radius, it may be assumed for arc or sine indifferently. The vernier and microscopes would be useless long before this limit could be approached.

‡ Let the reading of the first microscope be A°, and of the extremity of the major axis of the ellipse be B°, then the errors of the elliptic circle, when two microscopes of 90° distance are used, are

$$+ m''. \sin 2(\mathrm{A}-\mathrm{B})^\circ \text{ and } + m''. \sin 2\left\{90 + \mathrm{A} - \mathrm{B}\right\}^\circ$$

which destroy each other: when three microscopes are employed, the errors are,

$$+ m''. \sin 2(\mathrm{A}-\mathrm{B})^\circ, + m''. \sin 2(120 + \mathrm{A} - \mathrm{B})^\circ, \text{ and } + m''. \sin 2\left\{240 + \mathrm{A} - \mathrm{B}\right\}^\circ$$

which also destroy each other.

Hence, if a circle be originally well divided, and from any accident suffer such a change of shape as makes it an ellipse, or approximately so; though the centre work may be forced from its place, and the plane of the circle inclined to the axis, yet it is capable of performing good work, and this is the state in which Mr. Sheepshanks conceives the Cape mural to be.

Soon after the erection of the mural circle at the Cape Observatory, there were vague rumours of its performance being unsatisfactory; and Mr. Fallows, in letters to Mr. Jones and Mr. Troughton, complained grievously of the discrepancies of its readings, but without any distinct statement of their extent or nature. After a good deal of experience, however, he came to the conclusion, that the mean of the six readings was to be fully depended upon, apparently upon the following grounds: — 1. That the *horizontal point*, *i. e.* the midway point between the place of a star seen directly and that seen by reflexion, was the same, whether the star was high or low, to the north or south. This proves that the circle is symmetrical on each side of the horizontal diameter. 2dly, That the observed *differences* of the declinations of known stars agreed with the best standard catalogues; and 3dly, that the latitude of the Cape Observatory as determined by *β Hydri* above and below the pole, agreed exactly with that determined by using the stars of the *Berlin Ephemeris*. (The difference from that deduced by means of the catalogue of the N.A. may probably be attributed to the tables of refraction.) From an examination of the observations themselves, it is certain that the circle, pier, and microscopes, are exceedingly steady; the extreme differences in the place of any star, or in the horizontal point, when the mean of the six microscopes is used, scarcely ever exceeding 2″, and generally not being greater than 1″.

After the death of Mr. Fallows several of his paper-books were deposited at the Admiralty, and Mr. Sheepshanks was allowed by Captain Beaufort to inspect them. Along with some other experiments, a series of readings at every 10° was found (see the accompanying paper, Table I.), upon which the present examination is founded. To make these readings suit equal intervals of 10°, the *reduction* was first subtracted from each of the microscopes; the microscopes were then set at exactly 60° apart, by adding to, or subtracting from, each microscope a constant quantity, such, that the vertical columns, when added up, = 0; and, lastly, the opposite readings were classed together.* The result of these operations is exhibited in Table II. If the circle were perfect in its form and divisions, it is clear that the sum of each pair of opposite readings should now = 0; and as this is not the case, and as the mean of the six microscopes is supposed to shew the truth, the half sum may be considered as the error of the diameter. These half sums are

* It would enable a person to examine a mural circle with much greater ease if this were always done. The readings ought to be considered as those of three diameters, each of which is independent of the other two.

arranged under the heads of $\frac{A+B}{2}$, $\frac{D+E}{2}$, $\frac{C+F}{2}$, which are next to be considered.

In the first place, if the instrument be firm, and the derangement permanent and independent of gravity, the error of each diameter should be the same for any reading, and for that reading $+180°$, because the same divisions recur under opposite microscopes; and it will be seen that the readings $\frac{A+B}{2}$ are sensibly the same after 180° that they are after 0°, and that $\frac{D+E}{2}$, $\frac{C+F}{2}$, follow the same law. Also the error $\frac{A+B}{2}$ for any reading should be the same as $\frac{D+E}{2}$ for the same reading $+60°$, and also as $\frac{C+F}{2}$ for the same reading $+120°$; since the divisions which are under the microscopes A and B come under D and E when the instrument has been turned round 60°, and under C and F when it has been moved through 120°. Upon inspection they do appear to be *sensibly* the same, $\frac{A+B}{2}$ or $-5'',4$, the error of the diameter 0° to 180° being the same as $\frac{D+E}{2}$ opposite 60°, and not differing more than the errors of reading off from $-6'',1$, the value of $\frac{C+F}{2}$ found opposite to 120°. Hence it may safely be concluded, that the errors, from whatever cause they may arise, are permanent, and do not shift about with the revolution of the instrument. The circle, therefore, is not *flexible* or *loose*.

On running down any of these columns, it is scarcely possible not to notice the regularity of the error, which for $\frac{A+B}{2}$, as is readily seen, differs very little from $+5'',5 \times \sin 2\{\text{angle read off} - 45°\}$.* Now this is exactly similar to the error which would exist if the shape of the instrument was changed from a circle to an ellipse after it was divided; and as we know that the case in which it was packed *was let fall while raising from the hold of the ship* previous to landing, it seems reasonable to conclude that such an alteration of form did really

* The approximate curve being supposed to be an ellipse, which cannot be far from the truth, the positions of the major and minor axis were obtained by finding the readings, where the error would be 0, in each set of errors, and taking a mean of the different results. The major axis appears to be in the diameter $45° - 225°$. The value $5'',5$, in like manner, was determined by employing three observations about the maximum, for each partial result, and taking the mean of the whole. It did not appear worth while to adopt any more accurate method, as the readings are not to be relied upon to any great exactness, probably not much nearer than $1''$.

take place at that time. Table III. shews the near coincidence of this hypothesis with the actual errors.*

In this way we may ascertain the errors, which are similar to errors of division. The remaining errors can only arise from the varying position of the *centre of the divisions*, which must be carefully distinguished from the axis, or *centre of motion*. These are exhibited in Table IV., which is the same as Table II., after $\frac{A+B}{2}$, $\frac{D+E}{2}$, $\frac{C+F}{2}$ have been subtracted from the readings, A, B; D, E; C, F. The two columns of co-ordinates to the centre have been found as follows. The horizontal line joining the microscopes A and B has been assumed for the line of abscissæ. The ordinates are positive when the centre is above the line, which diminishes the reading of A and increases that of B. The abscissa is + when the centre approaches A, in which case the upper readings D and C are increased and the lower diminished. The centre will be found in three lines, the first parallel to the line joining the microscopes A and B, and distant from it, $\frac{B-A}{2}$; the second parallel to the line joining D, E, and distant from it by $\frac{D-E}{2}$; the third parallel to the line joining C and F, and distant from it by $\frac{C-F}{2}$: thus, we have three intersections for the position of the centre: $x = \frac{D+C}{2} \times \text{sec. } 30°$. To save trouble, y has simply been taken = B. The form of the curve thus traced out by the centre is exceedingly irregular and perplexing, but there is no appearance of any slipping, which would be shewn by a breach of continuity in the curve, or of riding, when the path of the centre would be in two lines parallel to the sides of the Y's. Upon examining the readings of the circle when employed in actual observation, the centre is always found pretty nearly in the position which is given by this curve, so that this error may be considered to be *fixed*, as well as the derangement of the circle itself, and all suspicion of slipping or riding, &c. be dismissed as groundless. By supposing the axis to be bent, and to work within two Y's of considerable length, or by supposing the pivot itself to have become elliptical, it may be possible to account even for this irregularity; but the research is one of some difficulty. To obtain a satisfactory solution it would perhaps be necessary to possess a set

* It can hardly be supposed that a fall should change a circle into an *accurate* ellipse, and the approximation is as satisfactory as under the circumstances can be expected. The circle may be of a pear or egg shape. If the shock took place when the circle was vertical and axis horizontal, it seems probable that the elongation would be in the diameter then vertical, and that the whole mass was carried below the former centre, *i. e.* the axis, which became excentric and crooked, or perhaps elliptic. The error is too large to be explained by an inclination of the plane of the circle to the axis, which would however produce a *similar* effect.

of transit observations, direct and by reflection, which we do not find in Mr. Fallow's MSS. No heavenly body, except the moon, can be sensibly affected by the slight deviation thus caused from the true meridian, which is indeed vexatious to the observer, by compelling him in all cases to read his minutes as well as seconds, but not otherwise important.* The centre may be made more steady, if the wandering arise from the crookedness of the axis, by shortening the bearings of the front Y's very much, and by fitting a new ring upon the further end of the axis, which may be adjusted to have its axis in the prolongation of the front bearing; or this end of the axis may be made to work against a point, in which way a new axis could be given perpendicular to the plane of the circle. Whether this or any similar plan be adopted, it seems certain that the Cape circle will give good results, although the separate microscopes are exceedingly discordant; and as it has been found that in every instrument, a wandering or excentricity of the centre is remedied by using opposite readings; that a change of form from the circular to the elliptic is not injurious, within reasonable limits, if the limb of the instrument continue to be a scale of equal parts, and if the proper number of microscopes be placed in fitting situations; and, finally, that even an inclination of the plane of the circle to the axis, will not invalidate the mean of the readings; we may conclude, that, with proper care and precaution, circles may be used successfully, even if somewhat deranged, when the original division has been carefully executed.

III. A list of stars observed with the moon at the Royal Observatory, Greenwich, March 1832.

		h m s	Daily Rate of Transit Clock.
March 9.	*m* Tauri	4 58 13,88	
	☽ 1 L.	5 10 53,88	
	119 Tauri	5 23 4,36	+0ˢ,15
	N Tauri	5 38 20,98	
10.	N Tauri	5 38 22,00	
	☽ 1 L.	6 13 7,34	+0ˢ,15
	ζ Geminorum.....	6 54 51,16	
11.	ζ Geminorum.....	6 54 51,32	
	☽ 1 L.	7 16 23,22	−0ˢ,14
	g Geminorum.....	7 37 6,54	
12.	*g* Geminorum.....	7 37 6,54	
	ζ Cancri	8 3 17,44	
	☽ 1 L.	8 19 20,90	+0ˢ,17
	o' Cancri	8 48 35,86	
13.	*o'* Cancri	8 48 35,86	
	☽ 1 L.	9 20 44,54	
	† ν Leonis.....	9 49 54,97	0ˢ,00
	Regulus.....	10 0 8,80	
15.	*l* Leonis.....	10 41 9,28	
	☽ 1 L.	11 16 15,70	0ˢ,00
	ν Virginis	11 37 57,42	

* This may be avoided by using a small table shewing the difference between A and the mean of the six microscopes at every 10°.

† At three wires only, *cloudy*.

IV. Transits of the moon with moon-culminating stars observed at Cambridge, in the month of May, 1832.

Day.	Name.	Time of Transit. h m s	Clock's losing rate. s
March 10.	N Tauri	5 37 22,07	1,30
	☽ 1 L.	6 12 7,15	
	ζ Geminorum	6 53 52,26	
11.	ζ Geminorum	6 53 50,89	1,30
	☽ 1 L.	7 15 21,77	
12.	*g* Geminorum	7 36 4,83	1,23
	ζ^1 Cancri	8 2 15,55	
	☽ 1 L.	8 18 18,03	
13.	☽ 1 L.	9 19 40,20	0,97
	♍ Leonis	9 48 51,25	
	Regulus	9 59 5,73	
15.	*l* Leonis	10 40 4,21	0,85
	☽ 1 L.	11 15 9,54	
	♍ Virginis	11 36 52,43	

Mr. Baily stated, in reference to Dr. Robinson's paper "On the dependence of a clock's rate on the height of the barometer," read in June last, that he had recently swung the mercurial pendulum in a vacuum apparatus, and found that a variation of one inch in the pressure of the atmosphere produced a difference of 0·42 seconds a-day in the rate of the pendulum; which is more than double the quantity deduced by Dr. Robinson from the *observed* alteration of rate when compared with the height of the barometer.

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